

SCIENCE

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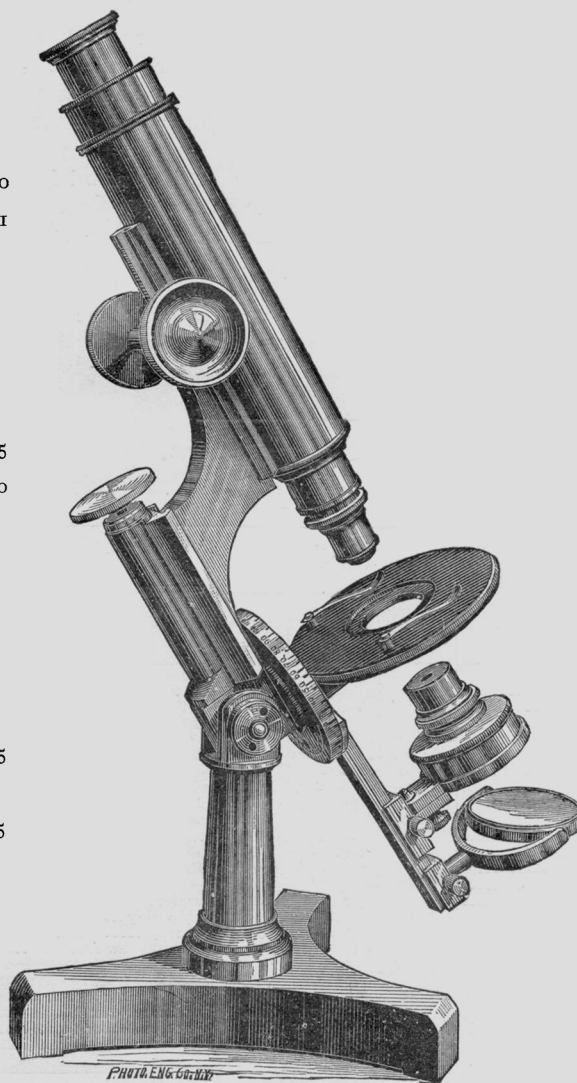
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SCIENCE:

A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS.

ILLUSTRATED.

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A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

PUBLISHED AT
229 BROADWAY, NEW YORK.
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SATURDAY, APRIL 16, 1881.

We present in another column a communication from Professor Alexander Winchell, of the University of Michigan, who, in response to a request from ourselves, has presented a very clear statement of his views relating to some conditions of primitive matter. This subject was introduced in "SCIENCE" by Mr. Larkin (March 26), and followed by Mr. Morris in our last issue.

Much difference of opinion exists on this subject, which is one of the highest interest, and we trust that the open discussion we have permitted may elicit some truth, and lead to a removal of many of the difficulties which underlie this question. Without anticipating our opinion on the merits of the respective arguments which have been adduced or the conclusions to which they lead, we may state that both Mr. Larkin and Mr. Morris, in speaking of gravity, magnetism, heat, light, electricity, motion, etc., etc., appear to make statements which do not accord with the standard authorities on these subjects.

That such difference of opinion should exist on what may be considered fundamental points, should cause no surprise, when even the nomenclature of the physical sciences is in a state of confusion. On this subject we refer our readers to an able article by Professor A. E. Dolbear, published in "SCIENCE" last November (Vol. 1, page 238); this paper demands the careful study of all who would take part in this discussion, and affords a basis on which it may be conducted with profitable results.

We make no apology for introducing this subject to our readers, especially as it has been recently mentioned in popular and scientific books, in connection with philosophical, ethical and theological questions. The objective point of Mr. Larkin's arguments appears to be directed against the Nebular hypothesis of Laplace.

Professor H. Helmholtz, whose lecture on this subject has been recently published by Messrs. Appleton & Co., makes a stout defence of this hypothesis. He asserts that "science is not only entitled, but is be-

holden, to make such an investigation. For her it is a definite and important question, as it involves the existence of limits to the validity of the laws of nature, which rule all that now surrounds us; the question whether they have always held in the past, and whether they will always hold in the future; or whether, on the supposition of an everlasting uniformity of natural laws, our conclusions from present circumstances as to the past, and as to the future imperatively lead us to an impossible state of things; that is, to the necessity of an infraction of natural laws, of a beginning that could not have been due to processes known to us."

As Mr. Helmholtz observes, to commence such an investigation as to the possible or probable primeval history of our present world considered as a question of science, is no idle speculation, for it is a question as to the limits of its methods, and as to the extent to which existing laws are valid.

We have received several interesting letters on this subject which will be found in our next issue.

THE SEA-SIDE LABORATORY.

The liberality and co-operation of the Woman's Education Association enables the Boston Society of Natural History to announce that a Sea-side Laboratory, under the direction of the Curator, and capable of accommodating a limited number of students, will be open at Annisquam, Mass., from June 5th to September 15th.

Annisquam is situated on an inlet of Ipswich Bay, on the north side of Cape Ann, and is about three and a half miles by coach from the Eastern Railroad Company's station in Gloucester.

The purpose of this Laboratory is to afford opportunities for the study and observation of the development, anatomy and habits of common types of marine animals under suitable direction and advice. There will therefore be no attempt, during the coming summer, to give any stated course of instruction or lectures.

It is believed that such a Laboratory will meet the wants of a number of students, teachers and others *who have already made a beginning in the study of Natural History*. Those who have had some limited experience in a laboratory, or who have attended the practical lessons given by the Teachers' School of Science of the Boston Society of Natural History, are sufficiently qualified to make use of this opportunity.

The work in the Laboratory will be under the immediate care of Mr. B. H. Van Vleck, Assistant in the Museum and Laboratory of the Boston Society of Natural History, a thoroughly competent instructor, and one who has also had long experience in collecting and observing at the sea-side.

Those who would avail themselves of this excellent opportunity to study living objects at the sea-shore should make application to Mr. Alpheus Hyatt, Curator of the Boston Society of Natural History.

THE AMERICAN CHEMICAL SOCIETY.

The April meeting of the American Chemical Society was held on the evening of the 1st inst. There not being a quorum present at eight o'clock, the business of the Society was postponed, and the reading of Dr. A. R. Leeds' paper on "Anilo-metallic Compounds" took place. The aniline compounds of Aluminum, Antimony, Barium, Calcium, Cadmium, Chromium, Copper, Cobalt, Bismuth, Mercury, Tin and Zinc were described; how they were prepared and their important characteristics noted. Aniline will not combine with any monivalent element. This paper was a preliminary report of work which Professor Leeds proposes to extend and ultimately publish when he shall have obtained sufficient data. The second paper on the "Action of Concentrated Sulphuric Acid on Lead Alloys" was read by Lucius Pitkin, one of the most talented young chemists of the School of Mines, N. Y. In a paper presented by Mr. James Napier before the Glasgow Philosophical Society, it was held that impure lead was preferable to the pure article for use when in contact with sulphuric acid (see *Chemical News*, Dec. 23, 1880). Mr. Pitkin tried the action of both hot and cold concentrated acids on some forty samples of lead and its alloys. The alloys treated were of lead with antimony, tin, bismuth, cadmium, silver and zinc. In the case of cold acid, 2 sq. in. of each alloy and a sample of pure lead were exposed for 24 hours to the action of 10 c.c. of sulphuric acid at 20° C.; on hot acid the length of exposure was one hour, and his results are best given in the following table, with which his paper terminated.

Average solubility or liability to formation of sulphate of the alloys in terms of lead.

		Cold Acid.	Hot Acid.
Lead		I.	I.
"	alloyed with Antimony.....	.81	2.75
"	" Tin	1.42	.75
"	" Bismuth.....	1.10	7.69
"	" Cadmium	.86	1.10
"	" Silver	.87	.93
"	" Zinc.....	1.53	1.10

Considerable discussion followed Mr. Pitkin's paper, in which Dr. Gallatin, Dr. Geyer, Dr. Alsberg, Mr. Herreshoff and Dr. Squibb participated.

Mr. A. E. Hoppick was then elected a regular member of the Society, and Messrs. C. P. Sawyer, A. H. Van Sinderen, and Otto Grote, were proposed for election. Mr. J. H. Stebbins was elected to fill the vacancy caused by the resignation of Dr. Gallatin, as Recording Secretary, and Mr. Herreshoff elected to the position on the Committee on Nominations which Mr. Stebbins had held.

Mr. Casamajor and Dr. Alsberg reported on behalf of the Committee for the Annual Dinner, and announced that the fifth anniversary dinner of the American Chemical Society would take place at Sieghörtner's restaurant, on Monday, April 18, at 6 P. M. M. B.

ON SOME PHENOMENA PRESENTED BY VORTEX-RINGS.

PROFESSOR A. E. DOLBEAR, TUFTS COLLEGE, MASS.

1. If one vortex-ring strikes another vortex-ring upon the edge the two rings will bound away from each other as though they were solid elastic bodies, each one vibrating as it recedes.

2. If one vortex-ring overtakes another ring, both moving in the same straight line, and both are of the same size, then the forward one will expand in diameter, and the latter will contract in diameter, and will go through the forward one when each will return to its original dimension. At the same time the forward one will have its velocity retarded while the other will have its velocity increased, and it may overtake the forward one and go through it.

3. If a vortex-ring passes near any light object as, for instance, a silk thread suspended, or better still a small cloud of smoke or ammonium chloride dust, the latter will be seen to be apparently repelled from the front of it but attracted and drawn into the ring from the rear.

4. If a vortex-ring be projected parallel with any surface, and at not too great a distance from the surface, the ring will move in a curved path towards it and strike it.

5. If two vortex-rings are projected so as to start in parallel lines near to each other they will approach each other until they touch, when they may be either broken or else bound away from each other as in the first case above.

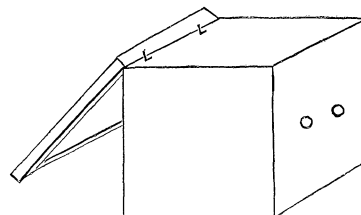
6. If two vortex-rings having the same rate of rotation be started in lines parallel to each other and at not too great a distance apart, they will not only approach each other but *they will combine to form one ring* which continues to move in the same direction.

7. The combination is effected by the breaking of each at the point of contact, and the welding of the opposite parts of each ring to form one ring with twice the diameter.

8. Three rings may in like manner be combined into one.

9. The structure of the vortex-ring is concentric, that is, a cross section of a ring generally shows a series of several concentric circles, with a hollow centre. The middle of the ring appears to be a cylindrical unoccupied space.

As experimental work with such rings is very entertaining as well as suggestive of the behavior of the real atoms of matter, it may be well to give the simple instructions necessary to perfect success.



Provide a cubical box with dimensions about a foot each way, having a swinging back frame, over which is stretched a piece of stout cotton cloth. On the opposite side two or more inch-holes may be bored two inches apart. Pour some strong hydrochloric acid into one saucer, and some strong ammonia water into another. Set the two into the box, and shut down the door. The box will at once be filled with the white fumes, and a tap with the finger upon the cloth back, will send out well-formed rings.

The phenomena 1 to 5, can best be seen by employing only one of the holes, so as to form but a single ring. By striking the cloth a little harder the second time than the first, the second ring may be made to overtake the first, and if it is desirable to exhibit the rings to a room full of people, there should be but a single hole in front, and that one about three inches in diameter; the rings can then be projected with force enough to make them go ten or fifteen feet from the box.

The other phenomena can best be studied by using only small holes, and tapping gently. The rings will come together within a few inches of the box. It seems to be essential that the two rings that combine, should have the same *rate of rotation*, a matter easily secured by forming the two at once in the above described way, but well nigh impossible, if one is formed after the other.

It is sufficient now to remark that the new phenomena described above simulate in a very striking way, what we call gravitation and chemism.

THE CO-EFFICIENT OF SAFETY IN NAVIGATION.*

By PROF. W. A. ROGERS, of Harvard University.

It is customary among engineers and architects, in making allowance for the strain to be borne by any part of a structure, to assign to the materials used, a strength sufficient to withstand a strain somewhat greater than the structure is ever likely to be subjected to. By experiment it can be found, for example, what is the "breaking load" of a wooden or iron beam of given dimensions, and an empirical law is established which will give us approximately the breaking load of any beam, when we know the dimensions, material, etc.; but in order to cover all possible differences which may exist in various beams, a *Coefficient of Safety* is either introduced into the formula itself, or is applied to the result obtained from the formula. This co-efficient should be large enough to cover, not only the largest possible deviation between experiment and theory, but also to meet all unforeseen emergencies, such as time and age inevitably bring.

Passing now to the consideration of the term "*Co-efficient of Safety*," as applied to navigation, it is our object to find the limits within which, under ordinary circumstances, a vessel can be located at sea, and then adduce some considerations which will enable us to form an intelligent judgment in regard to the range of error to which observations are liable. The quantity wanted is the average number of miles error in latitude and longitude, which we may fairly charge upon a single observation at sea, under ordinary circumstances. We have then to find the co-efficient by which this number must be multiplied in order to secure absolute safety, as far as safety depends upon human means and exertions.

By an examination of the "British Wreck Register" and the official inquiries made into the causes of disasters at sea, it will be seen that the ratio of loss compared with the increase of tonnage afloat, has for many years been steadily increasing. This inquiry is, therefore not an idle one. It is our purpose to examine only those causes of wrecks which, in a measure, seem to have escaped attention in official investigations. They are:—

I.—Wrecks produced by causes clearly beyond human control.

II.—Wrecks resulting directly or indirectly from over-insurance.

III.—Wrecks caused by the deviation of the compass.

IV.—Wrecks caused by errors of observation at sea.

The first inquiry is an important one, since, if we can find how many wrecks are beyond human control, we ascertain, at the same time, how many are *within* human control. The method of investigation is by the examination of records of Courts of Inquiry for twenty years. Between 1785 and 1813 no less than eight British ships were either wholly or partially disabled by lightning. Of course, vessels lost and never heard from should be added to this list. Between 1864 and 1869 we find from the insurance records that 9999 sailing vessels and 589 steamers, or a total of 10,588, were wrecked. Of this number, the end of 846 is entirely unknown, or one-eighteenth of the whole number. It is probable, therefore, that seven out of ten wrecks occur from preventable causes.

In regard to the second head, it is certain that more insured than uninsured vessels are lost, and in not a few cases it has been possible to convict shipowners of purposely destroying their vessels.

The compass problem is an intricate one, and has never been fully solved, though the researches of Flinders, Barlow, Scoresby, Airy and Harkness have done much to convert great uncertainty into tolerable certainty. The first observations on the variation were by Bond, in 1668.

It is well known that the variation of the needle is very irregular. There are yearly, monthly and diurnal inequalities, the diurnal variation being discovered by Graham, in 1722. But the complexity of the problem does not stop here. The tendency of the present time is to build iron ships, and all ships now have more or less iron in their construction. These ships become, to a greater or less degree, themselves great magnets. In wholly iron ships the uncorrected deviation of the needle often amounts to 50°, thus rendering it utterly useless. The Admiralty Law in regard to "swinging" for the variation of the compass is a very clear statement of the case. It reads as follows:

"As the deviation or error of the compass caused by local attraction of the ships becomes changed in amount by any change in the ship's geographical position, and may be entirely reversed in its direction by the ship's proceeding from the Northern to the Southern Hemisphere it is to be invariably tested by azimuth and amplitude observations at sea, and the ship is to be swung for ascertaining the change of error on arrival at a foreign station, and also once a year, and the same is to be inserted in the log book and sent to the Admiralty with the quarterly return for December."

The next important discovery in this connection was by Barlow, who found that all the influence of iron bodies exerted on the compass resides on the surface. This discovery paved the way for Airy's method of correcting compasses, which is by swinging the ship in the usual way, and then correcting the local attraction of the ship by means of permanent magnets of soft iron conveniently placed with respect to the compass. But the most important discovery was made by Dr. Scoresby. He found that every iron ship is itself a magnet, and that it gets its magnetism while building by the inductive magnetism of the earth, the poles of the ship's magnetism depending on the position of the building yard and the direction of the keel in construction. Dr. Scoresby made the voyage of the world in the *Royal Charter*, to test his theory, and found it fully confirmed. Before starting, his compasses were corrected by Airy's plan. On arriving at Melbourne, it was found that a complete inversion of the ship's magnetic polarity had taken place. Every stanchion, every standard, every davit, every mass of iron about the deck had in its upper surface acquired a Northern, instead of a Southern polarity, and the starboard compass had lost nearly one-half of its original errors. On returning to the place of starting in the Northern Hemisphere, and swinging the ship, it was found that a re-inversion had taken place, but the compasses did not quite return to their original deviations, but retained a fraction of their errors. It has since been found that these changes are much greater in steam than in sailing vessels, as shown by observations on board the *Vulcan* (steam), and the *Pandora* (sailing).

In 1852-53, Dr. Scoresby, in a paper before the British Association, showed that there is a sensible difference in the deviation before and after steam is up. It has been said that the compasses of a steam vessel, when light and running before the wind, with a high sea, are practically useless.

More recent experience has shown that the magnetism of an iron ship does not attain its normal condition till some twelve months after launching, and that for some time the variation is very irregular. In the *Great Eastern* a fixed compass changed its deviation nearly 3 points in the first 9 months of service. The observations of Prof. Harkness on board a monitor seem to be conclusive on this point.

We come now to the consideration of the fourth point. As early as 1598, Spain offered a reward of 100,000 crowns for the discovery of a correct method of finding the Longitude at sea. The States of Holland, at an early date, offered a reward of 100,000 florins, and France a reward of 100,000 livres. In 1714 the British Government offered a reward of £10,000 to any one who should

* Abstract of a paper read before the Naval Institute of Annapolis. Prepared under author's direction.

discover a method of finding the longitude at sea within 60 miles, £15,000 if with 45 miles, and £20,000 if within 30 miles. This offer did much to awaken interest in the subject. Though we have long since passed the lowest limit then mentioned, 30 miles, it is doubtful if any two navigators will agree as to what limit we have actually reached. The general testimony of sea-captains, in answer to my inquiries on this point, is that one mile is the ordinary limit within which the co-ordinates of a ship's place can be determined. A few placed the limit at half a mile. Only one navigator, with an experience of 30 years, placed the limit at 5 miles.

Two methods were proposed for the solution of the problem. Morin proposed what is now substantially the Lunar Method, and Maskelyne undertook the solution of the problem by observing the astronomical phenomena, such as eclipses of Jupiter's satellites. On the other hand, mechanicians devoted every energy to the mechanical problem. As the result of these labors, we have two essentially different methods for the determination of Longitude at sea.

I. By Lunar Distances, Occultations and Eclipses of Jupiter's Satellites, &c.

II. By Chronometers, assuming a rate at the beginning of the voyage.

The latter has for a long time been regarded as the more accurate method, but the difficulties to be overcome can be readily imagined, when we consider that even in the determination of the position of fixed observations, in which appliances of the utmost refinement are at hand, the places vary widely from the truth. For example, we find variations in the measured difference of longitude between Greenwich and Paris, as great as $5.5''$, or $1\frac{1}{4}$ miles, existing previous to the introduction of the telegraphic method of determining longitudes. The range between the earlier determinations of the difference of longitude between Greenwich and Brussels is 10 miles. Moon Culminations are more accurate than Lunars, but the same in principle. They are the more accurate when the longitude depends upon observations at each station, since the errors of Tables are thus eliminated. From a careful discussion of a long series of observations made at fixed observatories, with the most perfect instruments, it is found that we must expect from the Lunar Method an absolute error of six miles as the result of any number of observations. This corresponds in a general way with Prof. Peirce's investigation. He found that the ultimate limit, when one limb of the moon was observed, to be $0.55''$. "Beyond this," he says, "it is impossible to go with the utmost refinement. By heaping error upon error, it may crush the influence of each separate determination; but it does not diminish the relative height of the whole mass of discrepancy." But the discrepancy between the results for different limbs of the moon often amounts to $10''$ in the mean determination of a year. The assumption that the ultimate limit of accuracy is as great as $1''$ seems to be a very moderate widening of the limits. I find it to be $2.4''$.

For fixed observatories, using the moon's tabular place, we must expect an error of 3.1 miles, with a range of 12.9 miles. For Lunar Distances with sextant, on land, we must expect an error of 10.2 miles, with a range of 24.2 miles. For Lunar observations at sea these quantities should at least be doubled.

We now come to the subject of chronometers. The sources of errors are:

(a) Variations of rate arising from the action of magnetism. Airy's experiments show an extreme variation of $5.8''$ in the daily rate of a chronometer, due to terrestrial magnetism.

(b) When chronometers are swung on the same support, it is probable that there is a sympathetic action between them, similar to the results recently found by Mr. Christie with the Transit of Venus Clocks.

(c) Variation on account of change of barometric

pressure. This varies between $0.3''$ and $0.8''$ per day for every inch of change in the barometer.

(d) Variation between land and sea rates. Almost every chronometer will change its rate, when its circumstances, either of rest or motion, are changed. The Boston standard clock of Messrs. Bond & Son, almost invariably has a different rate on Sunday from any other day of the week. So, also, it has a change of rate when the streets are covered to any considerable depth with snow.

(e) Variation of rate at sea, on account of change of temperature. Mr. Hartnup, of Liverpool, was the first to give, not only the general rate of a chronometer, but also the rate for different temperatures.

An elaborate discussion of the errors of chronometers, from data collected at the Greenwich Observatory, from chronometric expeditions and from chronometers used in the Merchant and Naval services, the following result has been reached.

At the end of 20 days the navigator must expect an average error of 36 miles. He must look out for an error of 36×32 or 11.5 miles, and the amount of his error may prove to be twice this quantity, or 21 miles, all on the supposition that he has an average chronometer, and this is independent of the errors of observation, which must still be added.

We come finally to the consideration of the problem, —How near is it possible to find the place of a ship at sea by astronomical observations, taking into account all the errors to which observations are liable?

For the sake of simplicity we shall consider but one method, the method usually followed, viz.: by measurement of the altitude of the sun with a sextant, at a given time before it comes to the meridian for longitude, and the measurement of its culmination, for latitude.

We must first of all ascertain the magnitude of the errors to which observations with the sextant are liable. The following are some of the errors which we must ordinarily expect in observations with this instrument:

(a). Instrumental errors, such as eccentricity, errors of graduation, index error, &c. Errors of this class often exceed one minute of arc, even in a first-class instrument.

(b). Errors in noting time. No observer at sea pretends to note the time closer than one second. If we assume this low limit and multiply the co-efficient 3.5 already found, we have an error of nearly one mile.

(c). Errors arising from imperfect sea horizon.

(d). Errors arising from the use of approximate data.

(e). Errors depending on the latitude of the ship and the time of observation. By combination in the same direction, errors of this class may be very large, and, for the most part, they escape the attention of the navigator. The most favorable time for an observation of the sun for longitude is when it is exactly east or west. Here an error of one minute of arc in the observed altitude produces an error of the same amount in the resulting time, but if the observation is made 40 minutes from the meridian, an error of one minute in altitude may produce an error of 6 miles in the resulting position.

(f). Errors arising from the error in the estimated run of the ship between the morning and the noon observations.

The data for assigning a limit to the errors of observation with the sextant are as follows:

1. *Observations on Shore.* From a discussion of the observations by Williams in 1793, by Paine in 1831, by various observers at Willets Point in 1869, 1871 and 1872, by Hall and Tupman at Malta and Syracuse, and by Newcomb and Harkness at Des Moines, we find that for *latitude* the average error of observation with the sextant is $8''$, that the average range between the greatest and the least results of a given series is $36''$, the latter value having a range between $14''$ and $59''$. The coefficient comes out 4.4. For *time* the average error is $1.1''$, the range is $4.7''$, and the coefficient is 4.4.

II. Observations at Sea.

Under this head three distinct investigations have been made, as follows;

(a.) From an examination of the results obtained by chronometric longitude expeditions, we find that for a voyage of 15 days the average error is 5.3^s; the range between the greatest and the least results in each series is 18.0^s; the latter value has a range between 1.5^s and 55.0^s, and the coefficient is 3.4.

(b.) The longitudes of 36 stations have been determined by various British naval expeditions. The chronometers were rated at the Greenwich Observatory before starting, and the observations for time at the terminal stations were made in the usual way with the sextant. Evidently more than usual care was taken both with the observations and reductions. We find that the average difference between the results obtained by different chronometers is 4.4 miles with a range of 15.1 miles. The average range between the different results for longitude is 5.0 miles with a range of 31.6 miles. The average number of chronometers was 11, and the average duration of voyage was 11 days.

(c.) During the spring and summer of 1880 Officer W. H. Bacon, of the Cunard steamer "Scythia," kindly undertook for me a series of systematic observations from which the relative errors could be determined with considerable certainty. A complete series for a single day consisted of five sights at intervals of fifteen minutes, about 8 o'clock in the morning, five sights in the neighborhood of 11 o'clock, and five sights at the corresponding hours in the afternoon. Observations were also made when the ship was in known positions as often as possible.

This series of observations has an exceptional value on account of the conscientious fidelity with which the programme was adhered to and of the skill with which they were made. The relative errors were determined by comparing each position with the mean of the series, the rate being determined both from the morning and afternoon observations and from the log.

The results obtained are found in the following table:

LIMITS IN MILES.	Average Error from Observations at 9 ^h and 3 ^h .	Average Error from Log at 9 ^h and 3 ^h .	Average Error from Observations at 11 ^h and 1 ^h .	Average Error from Log at 11 ^h and 1 ^h .	Difference between Observation and Log at 9 ^h and 3 ^h .	Difference between Observation and Log at 11 ^h and 1 ^h .
	No. Cases.	No. Cases.	No. Cases.	No. Cases.	No. Cases.	No. Cases.
0.0-0.5	1	0	0	0	7	6
0.5-1.0	00	0	0	0	1	2
1.0-1.5	00	13	3	3	3	3
1.5-2.0	4	5	0	0	3	2
2.0-2.5	2	4	0	0	2	3
2.5-3.0	2	1	0	0	1	0
3.0-3.5	2	2	0	0	7	2
3.5-4.0	4	1	4	5	1	2
4.0-5.0	1	3	5	5	4	4
5.0-6.0	0	0	1	1	1	5
6.0-7.0	0	0	2	1	2	2
7.0-8.0	1	1	0	1	1	1
8.0-9.0	2	0	1	1	0	2
9.0-10.0	0	1	0	0	1	2
10.0-11.0	0	0	0	0	1	1
11.0-12.0	0	0	0	0	2	1
12.0+	1	1	0	0	0	0

QUERY.

A SUBSCRIBER would like to know the best method of mounting Triple phosphate crystals (dry) so as to tack them to the slide without interfering with definition. —Replies invited.

ON THE ACTION OF BACTERIA ON VARIOUS GASES.*

By F. HATTON.

The experiments were made to ascertain the nature of the action exerted by various gases on the life and increase of bacteria, and to observe what influence the bacteria had on the percentage composition of the gases. The bacteria were obtained by shaking fresh meat with distilled water. The aqueous extract was filtered and exposed to the air for twenty-four to thirty-six hours; it was always found to be full of bacteria. A small flask was half filled with mercury, filled up with the bacteria solution, and inverted in a mercury trough. The gas under examination was then passed up, a small glass vessel was introduced under the mouth of the flask, and the whole removed from the trough. The liquid was examined daily as to the condition of the bacteria, the sample being removed by a piece of bent glass tubing having an india rubber joint. After about a week the gas was pumped out by means of a Sprengle and analyzed. Atmospheric air was first tried. The bacteria lived well during the fifteen days of the experiment (T. 15° to 22°). A large absorption of oxygen took place, but it was not replaced by carbonic anhydride; in a second experiment (T. 25° to 26.50) 20 per cent. of the oxygen disappeared, and only 17 per cent. of CO₂ was formed. Pure hydrogen after fourteen days had no action on the bacteria; the gas contained 0.34 per cent. CO₂, 98.94 per cent. H. Pure oxygen after ten days was converted into CO₂ 29.98 per cent., O 70.02 per cent. A mixture of CO 46.94 per cent., CO₂ 1.27, O 1.27, N 50.51, was next tried after fourteen days; the gas contained CO₂ 17.77, CO 0.55, H 7.58, CH₄ 2.50, N 71.57. In all of the above cases the bacteria flourished well. Cyanogen was next tried. The solution of meat turned gradually to a thick black fluid. On the fifth day very few bacteria could be seen. From this time, however, they increased, and on the twelfth day were comparatively numerous. On the fifteenth day the gas was analyzed; it contained CN 5.35, CO₂ 57.59, O 2.24, N 34.79; a second experiment gave similar results. It appears, therefore, that cyanogen is fatal to bacteria as long as it exists as such, but that it soon decomposes into ammoniac oxalate, &c., and that the bacteria then revive, especially in sunlight. Sulphurous anhydride was next tried; the bacteria lived during the fifteen days: the gas contained CO₂ 7.87, O 0.00, N 2.13, SO₂ 90.10. Similar results were obtained with nitrogen, nitrous oxide, nitric oxide, carbonic anhydride, a mixture of H and O obtained by the electrolysis of water and coal gas. In all cases the bacteria lived well during the experiment. The author next experimented with a solution of urea (0.98 per cent.) and phosphate of potash (0.4 per cent.), sowing it with bacteria. The bacteria lived well during the fourteen days of the experiment; small quantities of gas were evolved containing 0.53 per cent. CO₂, 2.64 per cent. O, and 96.82 per cent. N. An experiment was made with spongy iron, air, and bacteria. On the fourth day, all the bacteria had vanished; the air was analysed on the fifth day, and consisted of CO₂ 0.26, O 0.00, and N 99.74 per cent. Experiments were also made with acetylene, salicylic acid, strychnine (10 per cent.), morphine, narcotine, and brucine; none of these substances had any effect on the bacteria. On the other hand, phenol, spongy iron, alcohol, and potassium permanganate were very destructive to these microscopic growths.

Mr. W. M. HAMLET said that these experiments confirmed some observations of his own. He had found that bacteria could exist in almost anything—in carbonic oxide, hydrogen, 1 per cent. creosote, phenol, methylamin, methylic alcohol, chloroform. Moreover, Crace-Calvert had shown that they could live in strong carbolic acid. In

* Read before Chemical Society, March 3, 81. This paper obtained for the author the Frankland Prize of £50 at the Institute of Chemistry.

reply to Mr. WARINGTON the speaker said that the acetic acid fermentation went on in the presence of chloroform.

Mr. KINGZETT called attention to the fact that the oxygen was completely used up when the meat infusion was placed in contact with air. He did not think the experiments represented the action of bacteria on gases or of gases on bacteria, but rather the effects of various gases on the mode and extent of ordinary putrefaction.

Dr. FRANKLAND expressed his satisfaction with the results obtained by the author in his laborious research. He must confess that these results had surprised him not a little. The fact that bacteria, which were real organisms and could not be shielded under the term putrefaction, lived and flourished in SO_2 , CO , CN , &c., seemed to him very extraordinary, and the question arose whether the germs to which infectious diseases were probably due were not similarly endowed with a power of great resistance to ordinary influences.

Mr. F. J. M. PAGE said that Dr. Baxter had proved that with some fever-producing liquids, their virulence was destroyed by chlorine and sulphuric acid, and that he had seen some experiments at the Brown Institution which led to the same conclusion; so it seemed that, at all events in some cases, the virulence of infective liquids was due to organic matter, essentially different from the bacteria observed by Mr. Hatton.

NOTES ON CHICKEN CHOLERA.

We observe in a recent number of the *Chemical News* that C. T. Kingzett, F. C. S., points out, that, in explaining the protective influence of repeated inoculations with the attenuated virus of chicken cholera, against the more virulent forms of this disease, Pasteur finds it "impossible to resist the idea that the microscopic germ which causes the disease, finds in the body of the animal conditions suitable to its development, and that to satisfy the necessities of its life the germ alters certain substances, or destroys them, which comes to the same thing, whether it assimilates them or whether it consumes them with oxygen borrowed from the blood."

So, again, in cases where complete immunity has been attained, the birds "no longer contain food for the germ."

More striking still is the following passage in reference to chickens which are born proof against cholera:—"Animals in this condition may be said to be born vaccinated for this disease, because the foetal evolution has not placed in their bodies the proper food of the parasite, or because substances which would serve as such food have disappeared while they were yet young."

Now whether or not we may be prepared to regard the said parasite as the direct cause of the disease, it is remarkable that the reasoning of Pasteur should have culminated in the conclusion upon which Liebig insisted with considerable power.

If we turn to Gregory's (3rd) edition of Liebig's "Animal Chemistry" (p. 205) we find the following passage:—"The condition which determines, in a second individual, his liability to the contagion, is the presence in his body of a substance which by itself, or by means of the vital force acting in the organism, offers no resistance to the cause of change in form and composition operating on it. If this substance be a necessary constituent of the body, then the disease must be communicable to all persons; if it be an accidental constituent, then only those persons will be attacked by the disease in whom it is present in the proper quantity and of the proper composition. The course of the disease is the destruction and removal of this substance: it is the establishment of an equilibrium between the cause acting in the organism which determines the normal performance of its functions and a foreign power by whose influence these functions are altered."

I repeat that to me it seems somewhat remarkable that the investigations and reasoning of two such eminent (and

in many matters diametrically opposed) thinkers should have culminated in the same conclusion as regards the conditions of the living body which subject it to, or protect it from, infection.

While, however, it can be readily understood how a profuse growth of parasites could quickly alter or destroy a comparatively large amount of substance—as, for instance, happens in ordinary putrefaction—it does not appear to me so easy to accept Pasteur's reasoning as to his so-called vaccination.

In this inflicted process an attenuated virus is introduced into the body of a chicken which becomes ill but does not die. It does not die because, if Pasteur be correct, the parasites do not sufficiently multiply. Why do they not multiply? It cannot be on account of the insufficiency of the pabulum, for in the large majority of cases where death results this seems to arise from the very profusion of the growth of the parasite when more freely introduced.

Can it be expected, therefore, that even, say, in three successive inoculations the substance which I have here spoken of as pabulum can be entirely removed or destroyed by the very limited number of parasites which are introduced by the inoculations, and which so soon perish in the body? I think this cannot be expected; but if it may be, then the particular substance or substances upon which the parasites prey must be extremely limited in quantity. After all, we are faced with the enormous difficulty of ascertaining the nature of such substance, and the further equally great difficulty of understanding why an undiscovered and undetermined substance should be entirely absent from the bodies in some animals and present in varying proportions in others.

Here we come in contact with the weakest point in the parasitic theory. The immunity from a second attack of an infectious disease of the class in question is simply inexplicable under the parasitic theory. We are forced back to an alternative theory, and that is one of which we at present only recognize the beginnings.

A NEW CORTICAL CENTRE.*

By GRAEME M. HAMMOND, M.D., NEW YORK.

Physician to the Department for Diseases of the Nervous System in the Metropolitan Throat Hospital.

Some six years ago there appeared in the *Centralblatt*, Nos. 37 and 38, a short communication by Betz, embodying an account of certain nerve-cells found by him in the cortex of a region of the brain which he newly named the paracentral lobule. This paper has probably aroused more general attention among neurologists than any other paper of recent times dealing with the structure of the cerebral hemispheres, and this, on account of the anatomical confirmation which the discovery seemed to furnish, of the localization doctrine based on the electrical stimulation of the cortex carried out by Hitzig and Fritzsche.

After localizing these cells chiefly in the paracentral lobule and the upper ends of the pre- and post-central gyri of man, stating them to be very few in number in the lower halves of these gyri, Betz proceeds to say, "the constancy of the occurrence of these cells, not only as regards the cortical layer, but also the special convolutions in which they are found, led me to direct my attention to that portion of the brain of animals, and particularly of the dog, on which latter Hitzig and Fritzsche obtained such brilliant physiological results. I refer to that lobule which bounds the sulcus cruciatus. Now I found in this very lobule in the dog, cells in similar nests and of a similar shape. With the dog as in man they are distributed in the fourth layer."

Engaged in a study of the ganglionic masses of the forebrain of the cat, an animal on which the experiments of Hitzig and Fritzsche have been repeated, and in which

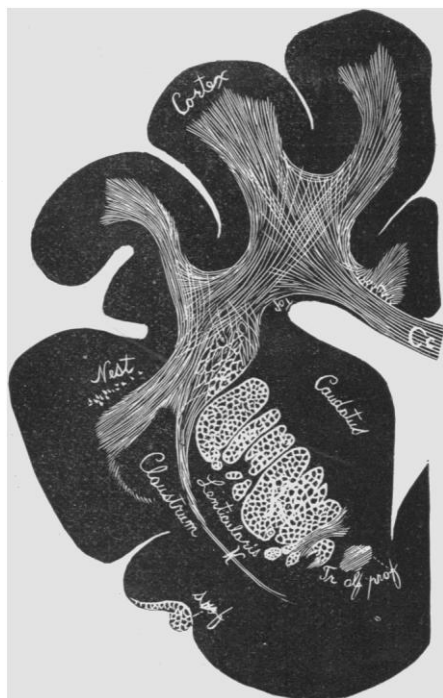
* Read before the New York Neurological Society, February 1, 1881.

the centres have been localized in regions homologous to those of the dog, and in which, furthermore, the architecture of the cortical surface is fundamentally the same, I proceeded to review the question of the localization of the giant-cells.

On the one hand, Betz seemed to argue that the giant-cells, which he claims to have discovered, were motor, because they were found at those points in the dog's brain where Hitzig and Fritsche, by supposed localized electrization, had produced contractions of special groups of muscles. Again, on the other hand, it is apparent that those interested in defending that narrow localization theory, which is such a prominent feature in the teachings of Charcot and Ferrier, have found one of their strongest supports in the anatomical discoveries of Betz.

Let us suppose then, for the sake of argument, that it be granted that larger cells mean motor centres for larger muscles; taking up the localizationists on their own ground we will examine the location of these giant-cells in a cat's brain, which only differs in a single exception from the dog's, and is therefore a fit subject for study. In fact, the cat's brain has the advantage of being somewhat simpler.

The results I have obtained are based upon the study of the cortical area of the two hemispheres of one cat. One hemisphere was cut as a whole into some seventy-five sections, from different altitudes transversely to the cerebral axis. The other was separated into eleven segments, and each segment cut into a number of thin sections. The series of sections derived from the first hemisphere served as a sort of topographical guide for the location of anything that might be found in the second.

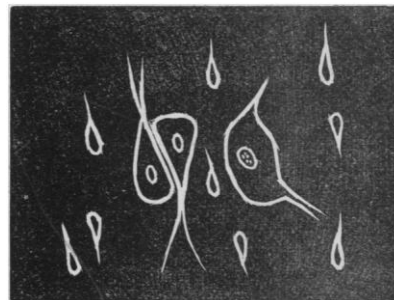


I found that the giant-cells are not confined to localized areas as Betz claims. I find that they are not as numerous near the sulcus cruciatus as they are much posterior to that region. I have even found giant-cells not very far from the base of the brain, but I found the largest group of the largest cells in a place not yet indicated on any of the charts of the localizationists as an unquestionable motor point. In the accompanying woodcut the position of the nest of cells that I have discovered is accurately demonstrated. These cells are ovoid, circular, and

sub-pyramidal in shape, and possess a round nucleolated nucleus situated about the centre of the cell. Each cell has from two to six visible processes. The ovoid cells are much the larger, their long diameter measuring from 0.08 mm. to 0.12 mm.; and their short diameter from 0.05 mm. to 0.06 mm. The circular and subpyramidal cells measure from 0.07 mm. to 0.08 mm. in diameter. The nuclei of all the varieties are of the same size, and measure 0.03 mm. in diameter. I only succeeded in finding them in one locality, but found them very numerous in that area. They are situated in the first primary arched gyrus, between the Sylvian and anterior Sylvian fissures. Ferrier, in his "Functions of the Brain" indicates a "centre" on the frontal division of the fourth external convolution, where, he says, he has observed, on irritating this centre, "a divergence of the lips so as to partially open the mouth." This centre approaches nearer in position to the one I have discovered than any other. With this study I was enabled to locate the chief foci for condensation of the giant-cells, of the shape known to Betz and Mierzejewski. These are pyramidal in shape, with a central round nucleus, and measure from 0.09 mm. to 0.12 mm. in length, and from 0.03 mm. to 0.04 mm. in width at the base. Their nuclei measure 0.02 mm. in diameter. The following woodcut shows how two of Betz's largest cells can be placed so that their conjoined areas are only equal to the areas of one of the ovoid cells such as I have described.

I regard the term "area of large cells" as inaccurate. The large cells are scattered more or less widely over the brain-surface, and it would be better to speak of "foci" when they are concentrated in larger numbers than elsewhere.

The giant-cell of Betz is not a new discovery. It is not a thing by itself distinct from the other pyramidal cells of the cortex. On the contrary, both in the human cor-



tex and in the cat, every transition from the average-sized cell of the third frontal layer to the giant-cell can be traced. I would also call attention to the fact that Betz states "these cells to be in nests" and not uniformly distributed in one layer, but I have seen, in one section from the paracentral lobule of the human brain, giant-cells arranged in regular order like soldiers on parade, for a distance of one-third of an inch.

Taking the deductions, which have been based upon the existence of these cells, on their merits, we find that those who have relied on this demonstration for the support of the theory of motor centres, are reduced to a number of predicaments. 1. That the largest giant-cells have been found in the brain of carnivora where no motor centre has been clearly demonstrated, and near which only small muscles are supposed to receive their cortical innervation. 2. That if, after all, this is a motor centre, that the method of localization was incompetent to detect it. I have limited myself this evening to this single fact. I need not say that the giant-cell was known to Meynert, although its locality was not accurately described by him. He claimed that the larger gyri of the frontal lobe contained the largest cells. On the other hand, cells as large as the giant-cells can be seen through the entire occipital lobe, according to this observer, in

the two white strata, and were described by him by the name of "solitary cells." I trust at no distant date to review the entire question of the distribution of large cortical cells with measurements and to submit them to the society.

For the present I think the existence of the large cortical cell group which I have described, shows conclusively that before the existence of large cells can be considered a demonstration of the correctness of functional localization, a more extended study must be made.

THE UNITY OF NATURE.

BY THE DUKE OF ARGYLL.

VII.

ON THE MORAL CHARACTER OF MAN CONSIDERED IN THE LIGHT OF THE UNITY OF NATURE.

(Continued).

It may be well, before proceeding farther in this branch of our inquiry, to retrace for a little the path we have been following, and to identify the conclusions to which we have been led.

In the first place, we have seen that the sense of obligation considered in itself—that is to say, considered apart from the particular actions to which it is attached—is a simple and elementary conception of the mind, inasmuch that in every attempt to analyze it, or to explain its origin and growth, this absurdity can always be detected,—that the analysis or explanation universally assumes the previous existence of that very conception for which it professes to account.

In the second place, we have seen that, just as Reason, or the logical faculty, begins its work with the direct perception of some simple and elementary truths, of which no other account can be given than that they are intuitively perceived, or, in other words, that they are what is called "self-evident," so in like manner the Moral Sense begins its work with certain elementary perceptions and feelings in respect to conduct, which arise out of the very nature of things, and come instinctively to all men. The earliest of these feelings is the obligation of obedience to that first Authority the rightfulness of which over us is not a question but a fact. The next of these feelings is the obligation of acting towards other men as we know we should like them to act towards ourselves. The first of these feelings of obligation is inseparably associated with the fact that all men are born helpless, absolutely dependent and subject to Parents. The second of these feelings of obligation is similarly founded on our conscious community of nature with other men, and on the consequent universal applicability to them of our own estimates of good and evil.

In the third place, we have seen that this association of the higher powers of Man with rudimentary data which are supplied by the facts of Nature, is in perfect harmony with that condition of things which prevails throughout Creation,—the condition, namely, that every creature is provided from the first with just so much of instinct and of impulse as is requisite to propel and guide it in the kind and to the measure of development of which its organism is susceptible, leading it with unfailing regularity to the fulfillment of the law of its own being, and to the successful discharge of the functions assigned to it in the world.

In the fourth place, we have seen that the only really exceptional fact connected with Man is—not that he has faculties of a much higher kind than other creatures, nor that these faculties are susceptible of a corresponding kind and measure of development—but that in Man alone this development has a persistent tendency to take a wrong direction, leading not towards, but away from, the perfecting of his powers.

In the last place, we have seen that as a matter of fact,

and as a result of this tendency, a very large portion of Mankind, embracing almost all the savage races, and large numbers of men among the most civilized communities, are a prey to habits, practices, and dispositions which are monstrous and unnatural—one test of this unnatural character being that nothing analogous is to be found among the lower animals in those spheres of impulse and of action in which they have a common nature with our own; and another test being that these practices, habits, and dispositions are always directly injurious and often even fatal to the race. Forbidden thus and denounced by the highest of all authorities, which is the authority of Natural Law, these habits and practices stand before us as unquestionable exceptions to the unity of Nature, and as conspicuous violations of the general harmony of Creation.

When, however, we have come to see that such is really the character of these results, we cannot be satisfied with the mere recognition of their existence as a fact. We seek an explanation and a cause. We seek for this, moreover, in a very different sense from that in which we seek for an explanation and a cause of those facts which have the opposite character of being according to law and in harmony with the analogies of Nature.

With facts of this last kind, when we have found the place into which they fit in the order of things, we can and we do rest satisfied as facts which are really ultimate—that is to say, as facts for which no other explanation is required than that they are part of the Order of Nature, and are due to that one great cause, or to that combination of causes, from which the whole harmony and unity of Nature is derived. But when we are dealing with facts which cannot be brought within this category,—which cannot be referred to this Order, but which are, on the contrary, an evident departure from it,—then we must feel that these facts require an explanation and a cause as special and exceptional as the results themselves.

There is, indeed, one theory in respect to those mysterious aberrations of the human character, which, although widely prevalent, can only be accepted as an explanation by those who fail to see in what the real difficulty consists. That theory is, that the vicious and destructive habits and tendencies prevailing among men, are not aberrant phenomena at all, but are original conditions of our nature,—that the very worst of them have been primitive and universal, so that the lowest forms of savage life are the nearest representatives of the primordial condition of the race.

Now, assuming for the present that this were true, it would follow that the anomaly and exception which Man presents among the unities of Nature is much more violent and more profound than on any other supposition. For it would represent the contrast between his instincts and those of the lower animals as greatest and widest at the very moment when he first appeared among the creatures which, in respect to these instincts, are so superior to himself. And it is to be observed that this argument applies equally to every conceivable theory or belief as to the origin of Man. It is equally true whether he was a special creation, or an unusual birth, or the result of a long series of unusual births each marked by some new accession to the aggregate of faculties which distinguish him from the lower animals. As regards the anomaly he presents, it matters not which of these theories of his origin be held. If his birth, or his creation, or his development, whatever its methods may have been, took place after the analogy of the lower animals, then, along with his higher powers of mind, there would have been corresponding instincts associated with them to guide and direct those powers in their proper use. It is in this essential condition of all created things that Man, especially in his savage state, presents an absolute contrast with the brutes. It is no explanation, but, on the contrary, an insuperable increase of the difficulty,

to suppose that this contrast was widest and most absolute when Man made his first appearance in the world. It would be to assume that, for a most special and most exceptional result, there was no special or exceptional cause. It Man was, indeed, born with an innate propensity to maltreat his women, to murder his children, to kill and eat his fellow, to turn the physical functions of his nature into uses which are destructive to his race, then, indeed, it would be literally true that

"Dragons of the prime,
That tare each other in their slime,
Were mellow music matched with him."

It would be true, because there were no Dragons of the prime, even as there are no reptiles of the present age—there is no creature, however terrible or loathsome its aspect may be to us, among all the myriads of created things—which does not pass through all the stages of its development with perfect accuracy to the end, or which, having reached that end, fails to exhibit a corresponding harmony between its propensities and its powers, or between both of these and the functions it has to perform in the economy of Creation. So absolute and so perfect is this harmony, that men have dreamed that somehow it is self-caused, the need and the requirement of a given function producing its appropriate organ, and the organ again reacting on the requirement and the need. Whatever may be the confusion of thought involved in this idea, it is at least an emphatic testimony to the fact of an order and an adjustment of the most perfect kind prevailing in the work of what is called Evolution, and suggesting some cause which is of necessary and universal operation. The nearer, therefore, we may suppose the origin of Man may have been to the origin of the brutes, the nearer also would his condition have been to the fulfillment of a law which is of universal application among them. Under the fulfillment of that law the higher gifts and powers with which Man is endowed would have run smoothly their appointed course, would have unfolded as a bud unfolds to flower,—as a flower ripens into fruit,—and would have presented results absolutely different from those which are actually presented either by the savage or by what is called the civilized condition of Mankind.

And here it may be well to define, as clearly as we can, what we mean by civilization, because the word is very loosely used, and because the conceptions it involves are necessarily complex. Usually it is associated in our minds with all that is highest in the social, moral, and political condition of the Christian nations as represented in our own country and in our own time. Thus, for example, respect for human life, and tenderness towards every form of human suffering, is one of the most marked features of the best modern culture. But we know that this sentiment, and many others which are related to it, were comparatively feeble in the case of other societies which, nevertheless, we acknowledge to have been very highly civilized. We must, therefore, attach some more definite and restricted meaning to the word, and we must agree to understand by civilization only those characteristic conditions which have been common to all peoples whom we have been accustomed to recognize as among the governing nations of the world. And when we come to consider what these characteristics are, we find that though complex, they are yet capable of being brought within a tolerably clear and simple definition. The Latin word *civis*, from which our word civilization comes, still represents the fundamental conception which is involved. The citizen of an imperial City,—the subject of an imperial Ruler,—the member of a great State,—this was the condition which constituted the Roman idea of the rank and status of civilization. No doubt many things are involved in this condition, and many other things have come to be associated with it. But the essential elements of the civilized

condition, as thus defined or understood, can readily be separated from others which are not essential. An extended knowledge of the useful arts, and the possession of such a settled system of law and government as enables men to live in great political communities, these are the essential features of what we understand by civilization. Other characteristics may co-exist with these, but nothing more is necessarily involved in a proper understanding, or even in the usual application of the word. In particular, we cannot affirm that a civilized condition involves necessarily any of the higher moral elements of character. It is true, indeed, that no great State, nor even any great City, can have been founded and built up without courage and patriotism. Accordingly these were perhaps the most esteemed virtues of antiquity. But these are by no means confined to civilized men, and are, indeed, often conspicuous in the savage and in the barbarian. Courage, in at least its lower forms, is one of the commonest of all qualities; and patriotism, under the like limitation, may almost be said to be an universal passion. It is in itself simply a natural consequence of the social instinct; common to Man and to many of the lower animals—that instinct which leads us to identify our own passions and our own sympathies with any brotherhood to which we may belong,—whatever the associating tie of that brotherhood may be,—whether it be morally good, bad, or indifferent. Like every other instinct, it rises on its moral character in proportion as it is guided by reason and by conscience, and in proportion as, through these, it becomes identified with duty and with self-devotion. But the idea of civilization is in itself separate from the idea of virtue. Men of great refinement of manners may be, and often are, exceedingly corrupt. And what is true of individuals is true of communities. The highest civilizations of the heathen world were marked by a very low code of morals, and by a practice even lower than their code. But the intellect was thoroughly cultivated. Knowledge of the useful arts, taste in the fine arts, and elaborate systems both of civil polity and of military organization, combined to make, first Greek, and then Roman, civilization, in such matters the basis of our own.

It is, therefore, only necessary to consider for a moment these essential characteristics of what we mean by civilization, to see that it is a conception altogether incongruous with any possible idea we can form of the condition of our first parents, or, indeed, of their offspring for many generations. An extended knowledge of the useful arts is of necessity the result of accumulation. Highly organized systems of polity were both needless and impossible before settled and populous communities had arisen. Government was a simple matter when the "world's gray fathers" exercised over their own children the first and the most indisputable of all authorities.

It is unfortunate that the two words which are habitually used to indicate the condition opposite to that of civilization are words both of which have come to mean a great deal more than mere ignorance of the useful arts, or a merely rudimentary state of law and government. Those two words are barbarism and savagery. Each of these has come to be associated with the idea of special vices of character and of habit, such as cruelty and ferocity. But "barbarian," in the classical language from which it came to us, had no such meaning. It was applied indiscriminately by the Greeks to all nations, and to all conditions of society other than their own, and did not necessarily imply any fault or failure other than that of not belonging to the race, and not partaking of the culture which was then, in many respects at least, the highest in the world. St. Paul refers to all men who spoke in any tongue unknown to the Christian communities as men who were "to them barbarians." But he did not associate this term with any moral faults, such as violence or ferocity; on the contrary, in his narrative of his shipwreck on the coast of Malta, he calls the

natives of that island "barbarous people" in the same sentence in which he tells us of their kindness and hospitality. This simple and purely negative meaning of the word barbarian has been lost to us, and it has become inseparably associated with characteristics which are indeed common among uncivilized nations, but are by no means confined to them. The epithet "savage," of course, still more distinctly means something quite different from rude, or primitive or uncultivated. The element of cruelty or of ferocity is invariably present to the mind where we speak of savagery, although there are some races—as for example, the Eskimo—who are totally uncivilized, but who, in this sense, are by no means savage.

And this may well remind us that, as we have found it necessary to define to ourselves the condition which we are to understand by the word civilization, so it is not less essential to define and limit the times to which we are to apply the word primeval. For this word also is habitually used with even greater laxity of meaning. It is often employed as synonymous with primitive, and this again is applied not only to all times which are prehistoric, but all conditions even in our own age which are rude or savage. There is an assumption that, the farther we go back in time, there was not only less and less extensive knowledge of the useful arts,—not only simpler and simpler systems of life and polity,—but also that there were deeper and deeper depths of the special characteristics of the modern savage. We have, however, only to consider what some of these characteristics are, to be convinced that although they may have arisen in early times, they cannot possibly have existed in the times which were the earliest of all. Things may have been done, and habits may have prevailed, when the multiplication and dispersion of Mankind had proceeded to a considerable extent, which cannot possibly have been done, and which cannot possibly have prevailed when as yet there was only a single pair of beings "worthy to be called" man and woman, nor even when as yet all the children of that pair knew themselves to be of one family and blood. The word primeval ought, if it is to have any definite meaning at all, to be confined to this earliest time alone. It has already been pointed out, that on the supposition that the condition of primeval man approximated to the condition of the lower animals, that condition could not have been nearer to, but must, on the contrary, have been very much farther removed from the condition of the modern savage. If, for example, there ever was a time when there existed on one spot of earth, or even on more spots than one, a single pair of human beings, it is impossible that they should have murdered their offspring, or that they should have killed and eaten each other. Accordingly it is admitted that cannibalism and infanticide, two of the commonest practices of savage and of barbarous life, cannot have been primeval. But this is a conclusion of immense significance. It hints to us, if it does no more, that what is true of one savage practice may possibly be true of others.

(To be Continued.)

ASTRONOMY.

COMPARISON STARS:—Under this heading Mr. Dreyer, in the last number of *Urania*, makes a most excellent "Suggestion to Astronomers" upon a matter which, of late, attracted some little attention. It is to be hoped that other observers will follow the example set at the Dunsink Observatory. Mr. Dreyer's "suggestion" is as follows:

"In spite of the numerous star-catalogues in the hands of observers of minor planets and comets, it frequently happens that a well-determined place for a comparison star cannot be found in any catalogue. Many stars have therefore to be re-observed, and much time is no doubt lost by a number of observers, each having to determine

the places of a few stars, which, if put together in one working list could be observed by one person with but little trouble.

It would evidently be an advantage if an astronomer, having at his disposal a good transit circle, would, for a time, endeavor to determine the places of all the comparison stars recently used and requiring re-observation.

In accordance with this scheme, I shall, until further notice (with the concurrence of Dr. Ball) be glad to determine with the Dunsink Transit Circle the places of any comparison stars north of -20° Declination not found in modern catalogues, and recently used in observations of minor planets or comets. The mean places, based on the Fundamental Catalogue of the 'Astronomische Gesellschaft,' will be worked out and published as soon as practicable."

THE SOLAR PARALLAX.

M. Faye has recently communicated to the Paris Academy of Sciences (*Comptes Rendus Tome XCII., No. 8*), an interesting paper upon the actual state of our knowledge of the sun's parallax. Remarking that there is no other constant in science whose determination depends upon such a large number of entirely independent results, he subdivides the various values assigned for the sun's mean parallax, as follows:

Geometrical Methods	{	8.85"	by Mars (Cassini's method).....	Newcomb
		8.78"	by Venus, 1769 (Halley's method).....	Powalky
		8.81"	by Venus, 1874.....	Tupman
		8.87"	by Flora, (Galle's method).....	Galle
		8.79"	by Juno.....	Lindsay
Mechanical Methods	{	8.81"	by the lunar inequality (Laplace's method).....	—
		8.85"	by the monthly equation of the earth.....	Leverrier
		8.83"	by the perturbations of Mars and Venus.....	Leverrier
Physical Methods	{	8.799"	velocity of light (Fizeau's method).....	Cornu
		8.813"	" " (Foucault's method).....	Michelson

In regard to the value 8.85" obtained by Cassini's method, M. Faye says that Mars has always given values for the solar parallax somewhat too large. The first value 8.81" obtained by mechanical methods was calculated by adopting for the coefficient of the inequality $125.2''$, the mean between the result of Airy from the Greenwich observations, and that of Newcomb from the Washington observations, taking for the moon's parallax $57' 2.7''$, and for her mass $\frac{1}{80} \times$. By the second of the "mechanical methods" Leverrier found 8.795, which was afterwards reduced to 8.85" by Stone upon correcting two slight errors in the computation. The value from the perturbations of Venus and Mars, assigned by Leverrier was 8.86", but one of the numbers requiring a small correction, it is reduced to 8.83". Michelson having overcome all the difficulties in Foucault's method, found for the velocity of light 2,999.40 kilom. $\pm$ 100 kilom. Using Struve's constant of aberration the corresponding values of the parallax are 8.799" and 8.813", as above. The general mean is 8.82", to which M. Faye attributes a probable error of $\pm 0.016''$. Although each of the values may be effected by systematic error, nevertheless, since the causes of error are varied, and without the least possible connection, these errors must be to a great degree eliminated, as well as the accidental errors.

The following conclusions are reached:

1. That the physical methods are superior to all others, and should be adopted.
2. That the value of the solar parallax, 8.813" (by physical methods), is now determined to about $\frac{1}{100}$ of a second.
3. That the seven astronomical methods converge more and more towards that value, and tend to confirm it, without equalling it in precision.

This fact does not diminish, however, the great importance of observations upon the coming transit of Venus, to which we can now bring to our aid the most effective of photographic apparatus.

W. C. W.

WASHINGTON, D. C., April 14, 1881.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

To the Editor of "SCIENCE":

PRIMITIVE STAGES OF COSMICAL EVOLUTION.

Some correspondence which has recently appeared in your columns seems to render it proper to offer a brief synopsis of my conception of the primitive modes of existence of cosmical matter. Supposing luminous cosmical matter to be intensely heated, there is no good reason for assuming this to be absolutely a first condition. It is undoubtedly a remote and primitive condition, which may be assumed as a stage from which cosmical development proceeds. But, in the light of recent science, we may reasonably seek for earlier stages at which cosmical matter existed in a cold and non-luminous state. In these stages we may conceive it either as atomically subdivided and dissociated, or as partly in a condition of molar aggregation, and perhaps of chemical combination. I am inclined to think, with Laplace and Kant, that space is abundantly stocked with cosmical matter in a crude and unformed state. I see no ground for assigning any limit to the state of subdivision in which some of it may exist. I see no satisfactory ground for assuming as Macvicar and others have done, that it emerges from an ethereal condition, nor for denying it. I do not imagine it to possess a temperature different from that of the space in which it floats—if, indeed space, which is not conditioned by matter, can be said to possess a property, which we only know as an affection of matter.

But there is a principle of gravitation in the universe, however we may explain it, which is ever beginning the aggregation of the ultimate elements of cosmical matter, and ever uniting these aggregations into masses larger and larger. We have made the acquaintance of some of these masses, large and small, in meteoric stones and millions of meteoroids, which become kindled only by friction with our atmosphere. It has been shown that several meteoroidal trains identify themselves with well-known cometary existences. It has been suggested that a rain of cosmical matter has pelted the minute satellites of Mars until the angular orbital motion of the inner one exceeds the axial motion of the planet. The collisions of molecules and masses must result in the evolution of heat. This, within certain limits of temperature, would promote the chemical union of atoms previously dissociated. But the impact of larger masses and groups of masses and molecules would undoubtedly develop sufficient heat to vaporize matter, and even to reproduce a state of dissociation. Such events would characterize the history of any vast region of space in which cosmical atoms should have become relatively approximated. They are in process of falling together. This is the condensation of the cosmical swarm. It may proceed, as Sir William Herschel conceived, until the successive stages of nebular condensation have been passed, when the swarm arrives at the condition of a sun. But it is well-nigh impossible for two cosmical masses to come together without producing rotation. The constituent portions of a nebula must probably rotate, and the nebula as a whole must, in most cases, exist in a state of rotation. Such a nebula was the starting point of Laplace, and was recognized as an actual contingency by Sir William Herschel.

But I do not conceive a fully formed and characteristic nebula as necessarily in a state of complete gaseity. The perpetual collision of hard parts might vaporize sufficient matter to occupy the intervening spaces and afford the characteristic spectral results.

As long as the nebula remains in process of condensation through central gravitation, the evolution of heat must continue. But there is a juncture at which rise of temperature must cease. That is reached when the elastic forces equilibrate the attractive. From this point, condensation develops heat, but some more is lost than is acquired, and the mass continually subsides in temperature. It is only the loss of an excess of heat which now permits the progressing condensation. Cooling is the limiting condition of condensation; and to assert a rising or a constant temperature is to assume that a condition may be surpassed by the effect of that which it conditions and measures. It is a virtual denial of the conservation of energy and of the equation of cause and effect.

In this view of cosmical beginnings, nebular heat is preceded by motion, and the cause of motion is what we call gravity. If, as Le Sage maintained, gravity is only the effect of the impact of a storm of ultramundane corpuscles, then our explanation ends at last in motion for which we cannot invoke gravity as an ultimate physical cause. Metaphysically speaking, such must be the issue of all explanations. There is a necessary *ultima thule* in the realm of thought. We are no nearer an absolute explanation at one stage of cosmical development than at any other. The remotest term reached must always stand scientifically unexplained.

I began by supposing luminous nebular matter intensely heated. There are many indications that such is its condition. The bright spectral lines and the analogy of the envelopes of the solar and stellar bodies are strongly suggestive. The rational continuity of cosmical development, leading our thoughts backward from an incrustated world through all conceivable stages, to an incandescent vapor or possibly gas, enforces the conviction of high nebular temperature. But, on the contrary, the very limited number of nebular bright lines spectroscopically revealed proclaims a fundamental condition widely different from that in the sun and fixed stars. It might be suggested that this indicates not only elemental dissociation, but an ulterior resolution, as Lockyer maintains, into one or two sole sorts of world-stuff; but it may also be suggested that the phenomenon is so divergent from the results of any terrestrial verifications that we are left without any substantial ground for inference. The nuclei of comets give also a few bright spectral lines. When the comet is near its perihelion it is not difficult to admit, in some cases, that the spectrum reveals a volatilized condition; but when a body so tenuous as to transmit star-light has retired from perihelion, it is difficult to believe that a gaseous condition is still the effect of high incandescence. The identification of cometary and cold meteoroidal trains, if it has truly been done, throws doubt on the assumed heat of even the nuclear portions of a comet remote from the sun; and yet even here it is supposable that perpetual collision of cold hard parts disengages sufficient heat to create a common gaseous medium. Finally, the meteoric streak of light left sometimes fifteen to thirty minutes after the fall and dissipation of the meteor, cannot be a case of heated luminosity. Heat vaporizes the meteoroid; but it is then a train of minutely divided particles exposed to almost instantaneous refrigeration. A few grains of matter strewn along a path twenty miles in extent, in the cold atmosphere, cannot retain luminosity as a consequence of high temperature. We might cite the streamers of the *aurora borealis*, and the Geisslerian discharges, and Crooke's radiant matter, and the general phenomena of phosphorescence as further reminders that intense heat is not the only cause of luminosity, and suggestions that nebular light may not be exclusively the light of thermal incandescence.

ALEXANDER WINCHELL.

UNIVERSITY OF MICHIGAN, April 8, 1881.

BOOKS RECEIVED.

SIGHT: An Exposition of the Principles of Monocular and Binocular Vision, by JOSEPH LE CONTE, LL.D., Professor of Geology and Natural History in the University of California.—D. Appleton & Co. New York, 1881.

This is another of those books specially prepared to bring within the reach of the general reader, some of the most interesting but complicated problems of science.

In the present case Professor Le Conte has attempted the double task of writing a work for the general public, which should also be found profitable reading for even the most advanced specialist. This would appear to be an embarrassing operation even with a popular subject, but with one so purely technical as optics the difficulties of the author must have been greatly increased.

To accomplish this programme with success, Professor Le Conte has divided his subject into two parts; the first treating, in an elementary manner, the anatomy and construction of the human eye, and the various theories involved in vision; and also a second part addressed to the specialist, in which some of the disputed points in Binocular vision are treated in detail.

Professor Le Conte, in speaking of the wonderful mechanism of the human eye, describes it as a masterpiece of Nature, whereas it is well known that many authors have been far less enthusiastic in its praise.

To show how differently two authors may treat the same subject even from the same point of view, let us compare what Professor Le Conte and Dr. H. Newell Martin says, as to the comparison of the human eye with a microscope objective, for curiously enough they both select the same object for this purpose, the former in the work now under review, and the latter in "The Human Body," a book recently received in our office, a notice of which is in preparation.

Professor Le Conte remarks, * "We see, then, that the mode of adjustment of the eye is somewhat like that of the Microscope." "Like the Microscope, but how infinitely superior."

Whereas, Dr. Martin observes: "The eye, though it answers admirably as a physiological instrument, is by no means as perfect optically; not nearly so good, for example, as a good Microscope objective."† Again Professor Le Conte, while indeed speaking of "defects of the eye as an instrument," refers such imperfections to a condition of disease or malformation, or at least to eyes which are not "normal or perfect," and specially mentions Myopia or Brachymetropia, Presbyopia, Hypermetropia and Astigmatism. Whereas, Dr. Martin points out defects of the human eye, which appear to be the normal condition of human vision, and exist "even in the best of eyes"—such as the "reflecting surfaces of our eyes not even being truly spherical, especially in the case of the cornea," and that "few persons are able to see equally clearly, at once, two lines crossing one another at right angles," this defect, when existing in a marked degree, causing serious trouble, amounting to disease, and known as "*astigmatism*."

In regard to the first part of the work, we may state that rarely has the subject been before treated in a manner so likely to realize in the mind of the student all that it is necessary to know in regard to the vision of man. The author's descriptions are so clear that to misconstrue them is impossible. Excellent illustrations are given, the majority being diagrams prepared by Professor Le Conte.

In regard to the more technical parts of the work relating to Binocular Vision, we find a difficulty in giving a resumé without diagrams, which it is not possible to present with this notice, but we trust we have aroused suf-

ficient interest in this book to induce those studying the question to make direct use of Professor Le Conte's work, where the whole subject is explained.

Professor Le Conte's own views, however, on Binocular vision may be given, which are expressed in the following words:

"All objects or points of objects, either beyond or nearer than the point of sight, are doubled but *differently*. The former homonymously, the latter heteronymously. The double images in the former case are united by *less* convergence, in the latter case by *greater* convergence of the optic axes. Now the observer knows *instinctively and without trial*, in any case of double images, whether they will be united by greater or less optic convergence, and therefore never makes a mistake, or attempts to unite by making a wrong movement of the optic axes. In other words, *the eye (or the mind) instinctively distinguishes homonymous from heteronymous images, referring the former to objects*, successive combination of the different parts of the object or scene, or pictures, as maintained by Brücke."‡

Professor Le Conte claims that this work "meets a real want, and fills a real gap in scientific literature;" in this assertion we heartily concur.

CORRESPONDENCE.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

A PROBLEM IN OPTICS.

To the Editor of "SCIENCE."

Will some of your correspondents answer the following questions, not from any tables given in books, but by original computation or observation. Given a plate of crown glass, index of refraction 1.525, with parallel surfaces, a ray of light incident on the under-surface 38° from the normal.

What will be the direction of the ray *in the glass*? What will be its direction of emergence from the upper surface into air? What its direction into water? What percentage of the light will be lost by reflection from the under surface of the plate? What will be lost from the upper surface?

If the light is incident 45° from the normal, what is the answer to each of the above questions?

As each color of the spectrum has a different degree of refraction the medium ray should as usual be taken for computation.

The thickness of the plate cannot affect the refraction *in* the glass, only the point where the ray reaches the upper surface. CARL REDDOTS.

OZONE is absorbed in a solution of arsenious acid to which a little neutral potassium iodide has been added. The excess of arsenious acid is estimated with standard iodine solution. 1. Ozone is a constituent of the higher atmosphere to a much larger extent than near the earth's surface. 2. Ozone is destroyed by contact with the gases and organic matter in a moist atmosphere, as near the earth's surface, and that the collection of ozone from the air is attended by the destruction of it to a large extent. 3. The absorptive power of the ozone in the air is quite sufficient to account for the limitation of the solar spectrum. 4. The blue tint of the atmosphere is largely due to ozone.—W. N. HARTLEY.

ERRATA.

How to obtain the Brain of the Cat, (Wilder).—Correction: Page 158, second column, line 7, "grains," should be "grams;" page 159, near middle of 2nd column, "successily," should be "successively;" page 161, the number of Flower's paper is 3.

* Sight, &c., page 45.

† The Human Body, page 502.

‡ Page 151.

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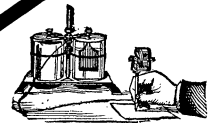
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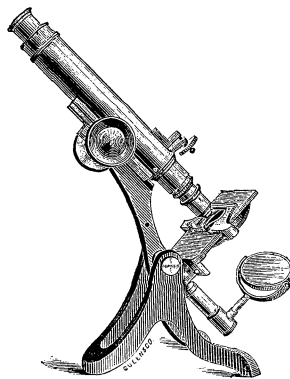
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CONTENTS.

Cosmical Evolution (Edit.); The Sea-Side Laboratory; American Chemical Society; On some Phenomena presented by Vortex-Rings, by Prof. A. E. Dolbear; The Co-efficient of Safety in Navigation, by Prof. W. A. Rogers; Notes on Chicken Cholera; On the Action of Bacteria on Various Gases; A New Cortical Centre, by Graeme M. Hammond; The Unity of Nature, VII., by the Duke of Argyll; Primitive Stages of Cosmical Evolution, by Prof. Alexander Winchell; Books Received; Correspondence; Notes, Astronomical, Chemical, &c., &c., &c.

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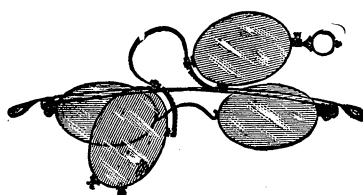
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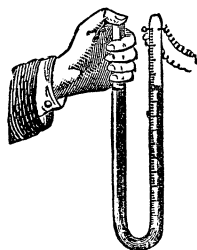
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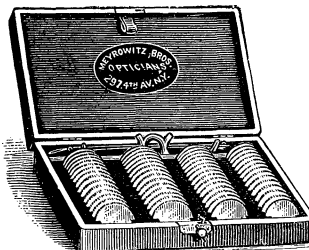


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